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Abstract:

Laboratory experiments are used to study the voluntary provision of a pure public good in the presence of an anonymous external donor. The external funds are used in two different settings, lump-sum matching and one-to-one matching, to examine how allocations to the public good are affected. The experimental results reveal that allocations to the public good under lump-sum matching are significantly higher, and have significantly lower within-group dispersion, relative to one-to-one matching and two baseline settings without external matching funds. In addition, a comparison of the two baseline conditions reveals a positive framing effect on public goods allocations when it is explicitly revealed to subjects that an outside source has made an unconditional allocation to the public good.

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Matching Contributions and the Voluntary Provision of a Pure Public Good: Experimental Evidence

1. Introduction

Laboratory experimental research on the provision of public goods has focused primarily on decision making in what is referred to as the voluntary contributions mechanism (VCM). In the most standard VCM decision setting, a group is comprised of a fixed number of individuals. Each individual is endowed with resources that can be allocated to either a private good that benefits only the individual (the private account) or to a pure public good that benefits all members of the group (the group account). The benefits are structured so that group earnings are maximized if all endowed resources are allocated to the group account. Each individual, however, has an incentive to free ride on the group-account allocations of other group members by allocating their resource endowment to the private account.

One topic addressed in the experimental public goods literature is institutional arrangements that reduce collective action problems by creating incentives that facilitate cooperation. The research reported here examines voluntary contributions to a public good in the presence of an external source of resources that are used for matching the contributions of group members. Two matching settings are examined. In the first, referred to as lump-sum matching, a publicly announced fixed level of resources from the external source flow to the group account only if the internal contributions of group members reach or exceed a pre-announced threshold level. In the second, referred to as one-to-one matching, each resource unit contributed to the group account is matched by the external source up to a publicly announced maximum level. Undertaking a controlled laboratory comparison of these alternative matching-fund settings is motivated by the observation that both arrangements are commonplace in fund drives for the provision of public goods in field settings (e.g. public radio fund drives).[1] The two settings with

[1] See Shang and Croson (2006) for a discussion of field experiments specifically linked to on-air public radio fund drives, as well as a review of other related studies.

matching are contrasted with two control settings without matching where external funds are allocated to the group account regardless of internal contributions. One control setting explicitly frames the unconditional contribution as a specific amount coming from an external source, and the alternative control setting simply adds, without explanation, the earnings generated by the external tokens to the payoff table for the group account when internal token allocations are zero.

These changes in experimental settings can be thought of in the following way. Assume a public good is to be partially funded through voluntary contributions. Further assume that the fund drive organizers have prior funding commitments that can be used for matching other potential donors' contributions. From the perspective of agencies receiving contributions, the strategic question is what type of institution makes best use of the matching funds. As discussed below, in the standard VCM environment matching funds create incentives where equilibrium strategies exist that imply non-zero provision of the public good.

The free-rider problem is particularly relevant for charitable giving, volunteerism, and other forms of philanthropy. While some of these activities can no doubt be rationalized as privately optimal, and in this respect no different from other economic activities, a significant amount of these activities entails personal sacrifices in order to improve social outcomes. This research is informative about the origin of such behaviors and their maintenance within social groups, since experiment participants experience similar incentives, albeit in a more abstract setting. By focusing on such a setting, the effect of economic incentives per se is investigated and comparisons are made that control for other factors that may affect behavior. In this context, the research reported here studies the role of alternative philanthropic institutions for promoting charitable contributions and explores how such institutions affect individual incentives, behavior, and resulting group outcomes relative to a known socially optimal outcome that maximizes the group's monetary earnings.

The paper is organized as follows. Section 2 summarizes related literature. Section 3 provides details of the experimental design and procedures. Section 4 presents experimental results, and conclusions are offered in Section 5.

2. Related Literature

There is a substantial literature in experimental economics studying the linear VCM decision setting. The stylized facts emerging from this type of experiment are that contributions to the group account exceed the standard economic prediction of zero tokens, but are below the socially optimal level of 100% percent contributions. There is, however, considerable heterogeneity across individuals in their choice of contributions and across decision making settings where group size and the relative payoffs of the public good to the private good are varied. (See, for example, Ledyard [1995] and Isaac et al. [1994].)

Because outcomes in public goods settings have tended to be sub-optimal, researchers have investigated ways to foster cooperation through, for example, face-to-face communication, sanctions, and rewards. In addition, several scholars have investigated institutional changes that relate more directly to the research reported here. Eckel and Grossman (2003) examine charitable contributions in the context of a one shot, individual choice environment, referred to as a "modified" dictator game. Given endowments, subjects choose a contribution level to actual charities under alternative subsidies. Rebate and matching mechanisms are investigated that, under suitable parameterizations, are functionally equivalent. Holding monetary incentives constant, gross contributions are greater in the case of matching. One explanation for this phenomenon is purely framing; subjects may view the act of contributing with matching in a more favorable context than a rebate, leading to greater overall contributions.[2] More recently, Karlan and List (2006) report the results of a field experiment examining the impact of one-to-one matching funds on contributions to a non-profit organization. Their design utilizes 1-to-1, 2-

[2] See Davis (2006) for further research related to the impact on charitable contributions of subsidies versus matching funds.

to-1, and 3-to-1 matching ratios. They conclude that matching increases both the probability of contributing and the magnitude of contributions, but variation in the matching ratio does not have a significant impact on contributions.

List (2006) provides a review of additional field experiments devoted to charitable giving. One such study relevant to the research reported here is Landry et al. (2006). The authors conducted a door-to-door fundraising experiment with contributions to a public good solicited in four treatment conditions: a standard VCM setting, a VCM setting with seed money, and two lottery conditions where subjects purchased raffle tickets: one with a single fixed cash prize, the other with multiple fixed cash prizes. Overall contributions to the public good ranked (from highest to lowest), multiple prize lottery, single prize lottery, VCM with seed money, VCM. In addition, the investigation into potential framing effects of the control setting in this study is closely related to a strand of existing VCM literature relating to "leadership" contributions. This literature examines the extent to which leadership contributions to the public good that occur early in the experiment can have a positive impact on the level of contributions; see for example, Rose-Ackerman (1986), List and Lucking-Reiley (2002), List and Rondeau (2003), Gachter and Renner (2004), Andreoni (2006), and Potters et al. (2007) .

Finally, from the perspective of strategic behavior, the literature on provision-point public goods relates closely to the lump-sum matching setting investigated here. See Marks and Croson (1998) for a review of this literature. The addition of a provision point to the VCM decision setting designates a publicly announced minimum level of resources that must be allocated to the public good in order for the public good to yield a positive return. If the provision point is not met, a refund condition is specified. Under a no-refund condition, if the provision point is not met any contributions to the public good are lost and yield no return to the contributors. In contrast, under a full-refund condition, contributions are returned when the provision point is not met. If the provision point is exceeded, a rebate policy must be specified for how such contributions will be used. The provision-point setting leads to multiple Nash equilibria. While all individuals allocating zero resources to the group account remains a Nash equilibrium, the group income-maximizing Nash equilibrium is to meet the provision point exactly. Nevertheless, exactly reaching the provision point can be achieved by multiple combinations of individual allocations. This implies a distributional conflict across subjects, where some subjects may attempt to free ride on the allocations of others.

3. Experimental Design and Procedures

3.A. The Decision Settings

This study incorporated four decision settings: lump-sum matching, one-to-one matching, and two no-matching baselines. All decision settings utilized variations of the VCM framework of Isaac et al. (1994), henceforth referred to as the standard VCM setting. Individuals made decisions in fixed groups of size N. At the start of each round, individual i was endowed with Z_i tokens which were divided between a private account, earning a constant return of p_i per token, and a group account, earning a return based upon the total number of tokens allocated by the group. Tokens could not be carried across rounds. For a given round, let m_i represent individual i's allocation of tokens to the group account and $\sum m_j$ represent the sum of tokens placed in the group account by all other individuals ($j \neq i$). Each individual earned $[G(m_i + \sum m_j)]/N$ cents from the group account. Because each individual received a 1/N share of the total earnings from the group account, the group account was a pure public good. At the end of each decision round, subjects were informed of their group's allocation to the group account, as well as their earnings for that round. Subjects were not informed of the individual decisions of group members.

The experiments were parameterized with subjects in groups of size N = 4 and individual endowments of 25 tokens per round. The return from each individual's private account was one cent per token, and the group's return from a token placed in the group account was $G'(\cdot) = 2.4$ cents. Defining the marginal per-capita return from the group account (MPCR) as the ratio of private monetary benefits to private monetary costs for moving one token from the private account to the group account yields MPCR = $G'(\cdot)/N = 0.60$.

Under the assumption that it is common knowledge that subjects maximize own-earnings and play a finitely repeated game with a commonly known end point, the sub-game perfect non-cooperative Nash equilibrium in this standard VCM setting is for each subject to allocate zero tokens to the group account. As discussed below, however, the settings that incorporate matching funds have important consequences for equilibrium predictions. Finally, note that the payoff dominant Pareto optimum in the standard VCM setting, and for all decision settings investigated in this study, is for subjects to allocate all tokens to the group account.

Lump-Sum Matching

In addition to the instructions for the standard VCM setting, subjects were informed that if total allocations to the group account met or exceeded 60 tokens, the group account would automatically have an additional 60 tokens added to it from an "external source" of tokens, with the earnings from these additional tokens being identical to those allocated by group members.[3]

Lump-sum matching creates a discontinuity in the payoffs associated with the group account at the point where the subjects meet the minimum threshold of 60 tokens. This property of the payoff function implies strategic elements to the game that lead to alternative Nash equilibria. In particular, similar to experiments with provision points, there are now multiple Nash equilibria. While all individuals allocating zero tokens to the group account remains a Nash equilibrium, the group income-maximizing Nash equilibrium is to meet the lump-sum matching threshold exactly. Thus, the symmetric Nash equilibrium is 15 tokens from each group member, but any other (asymmetric) combination of group-account allocations that exactly meet the lump-sum match threshold is also a Nash equilibrium. From a non-cooperative perspective, subjects have an incentive to free ride on the allocations of others if they expect others to allocate

[3] Subjects were explicitly informed that the external source was a computerized robot player, and loaded words such as "donor" or "contributor" were not used to describe the external source. Similarly, tokens were "allocated" to the group account, rather than "donated" or "contributed".

sufficient funds to the group account to meet the lump-sum matching threshold. On the other hand, from a game theoretic perspective, the symmetric Nash equilibrium of 15 tokens per group member may serve as a focal point for subjects (see Marks and Croson [1998]).

It is important to note a key difference between this setting and the provision point setting discussed above. In the lump-sum setting, if allocations to the group account do not meet the minimum requirement of 60 tokens, those tokens are still utilized as group-account allocations and generate earnings for the group. In the provision-point environments studied to date, if group account allocations do not meet the provision point, those tokens are either refunded to the private account or lost, depending upon the particular setting under investigation.

One-to-One Matching

Subjects were informed that each token allocated to the group account, up to a group maximum of 60, automatically led to an additional token being added to the group account from an external source. The group account earnings generated by each additional external token was identical to those internally allocated by the four group members.

The experiments with one-to-one matching create an increase in the marginal gain from allocations to the group account up to the maximum level of matching. Since the experiment is parameterized with an MPCR = 0.6, one-to-one matching implies an MPCR of 1.2 for group-account allocations up to 60 tokens. This property of the payoff function implies the existence of multiple Nash equilibria. In particular, an allocation to the group account that is matched yields a marginal return to the group member above the $0.01 per-token opportunity cost. In this setting, all group members allocating zero tokens to the group account is no longer a Nash equilibrium. As with lump-sum matching, there are multiple Nash equilibria where group members' total allocations to the group account exactly meet the maximum level of matching, and the symmetric equilibrium may serve as a focal point. From a non-cooperative perspective, subjects have an incentive to free ride if they expect others' group-account allocations to be sufficient to extract the maximum level of matching funds.

Note that the earnings consequences of some allocations in the one-to-one setting differ substantially from those in the lump-sum setting. In particular, in both settings subjects face the problem of coordinating over whom will provide the group-account allocations to be matched. The penalty, however, for not meeting the full-match threshold in the lump-sum setting is larger than in the one-to-one setting. In the lump-sum setting, the penalty is $0.36 per individual, regardless of how close the total group allocation is to the threshold. In the one-to-one setting, the penalty per individual is $0.006 for each token the group falls short of the maximum level of matching. Thus, falling a few tokens short of the threshold in the lump-sum setting has a relatively large negative effect on earnings, while an identical group-account allocation in the one-to-one setting has a much smaller effect. Focusing on this difference in the group-account earnings functions leads to the conjecture that lump-sum matching will generate greater group-account allocations than one-to-one matching. On the other hand, if group members in the one-to-one setting realize that matching results in the marginal private benefit of a token allocated to the group account exceeding the marginal private cost (MPCR = 1.2), an alternative conjecture is that the one-to-one setting will lead to a higher level of group-account allocations. Thus, standard theoretical considerations do not yield a clear prediction as to differences across the two settings in regard to the level of allocations to the group account.

Because of payoff differences that can occur within groups, the analysis of experimental outcomes will also focus on within-group dispersion of allocations to the group account. Both the lump-sum setting and the one-to-one setting lead to multiple equilibria that can support within-group dispersion in allocations to the group account, and subsequent subject payoffs. Given the severe penalty for not meeting the match in the lump-sum setting, however, the group allocation of 15 tokens per subjects may serve as a stronger focal point in this condition than in the one-to-one setting. Based on this consideration, one might expect to observe smaller within-group dispersion of allocations to the group account in the lump-sum setting than the one-to-one setting.

No-Matching Baselines

In addition to the two settings with external matching funds, allocation decisions from control groups without matching funds were also obtained. The earnings opportunities in these no-matching baseline settings paralleled those in the matching-fund settings, but without the strategic elements related to matching. The first baseline setting can be interpreted as framing the external tokens in a manner similar to a "leadership" contribution. All group members received a message that in each decision round an external source would allocate 60 tokens to the group account regardless of the group members' internal allocations. Thus, the minimum possible group earnings from the group account was 60 x $0.024 = $1.44.

A potential consequence of presenting the baseline setting in this frame is that subjects could be influenced by the external-source allocation to increase their own allocations to the group account. To investigate whether framing the additional 60 tokens as coming from an external source may have affected group members' allocation decisions, an alternative baseline setting was also implemented. In the alternative baseline, group members were not given a message regarding the source of the external tokens; they simply observed an earnings table that associated $1.44 with zero tokens allocated to the group account, instead of $0.00 when zero tokens were allocated.

The theoretical predictions for both baseline settings are identical to the standard VCM setting. Based purely on pecuniary gains, the sub-game perfect Nash equilibrium is zero tokens allocated to the group account. Thus, standard theoretical considerations suggest that both the lump-sum setting and the one-to-one setting are expected to yield higher allocations to the group account than the baseline settings. Further, due to the existence of multiple equilibria, both the lump-sum setting and the one-to-one setting are expected to yield greater dispersion of within-group allocations to the group account than the baseline settings. However, a large number of experiments examining the VCM setting report behavior that varies significantly from that predicted by standard theoretical considerations. Based on this evidence, it is an open question whether the baseline settings will yield smaller allocations to the group account and smaller within-group dispersion relative to the two settings that incorporate matching funds.

3.B. Procedures

[Table 1 here]

[Figure 1 here]

Table 1 and Figure 1 summarize the key elements of each decision setting. Each experimental session utilized twelve subjects who were randomly assigned to three four-person groups in each of three phases within a session. Subjects participated in a sequence of ten (phase-one) decision rounds in a particular setting, were then randomly reassigned to a new four-person group for ten (phase-two) decision rounds using a different setting, and were then randomly reassigned to another four-person group for the final ten (phase-three) decision rounds using a different setting. Each phase corresponded to a specific decision setting (baseline, lump-sum matching, or one-to-one matching) and the order of experimental settings was systematically varied across sessions. Thus, data on nine four-person groups were collected in each 12-person experimental session: three groups in each of the three phases, yielding three replications of a particular ordering of decision settings.

The experiments were conducted using NovaNET software at the Interdisciplinary Experimental Laboratory at Indiana University-Bloomington during the 2004-2005 academic year. Subjects were recruited from a database of volunteers.[4] After being seated at microcomputer workstations, subjects were given preliminary instructions that were projected on a large screen at the front of the room and read aloud by the experimenter.[5] Before beginning the first ten-round decision-making phase in the session, subjects were informed publicly that: 1) the experiment would consist of thirty decision rounds that were broken down into three ten-round

[4] A representative from the lab visited various large introductory classes (psychology, geography, and economics) to ask students to enlist in the database if they were interested in participating in experiments. A wide variety of majors are represented in these large introductory classes.
[5] Instructions are available upon request.

sequences, 2) for each ten-round sequence they would be randomly reassigned to a four-person group, 3) earnings at the beginning of each ten-round sequence would be displayed on their computer screen as zero, but, 4) their final earnings would be the sum of earnings across all three ten-round sequences, plus a $5 payment for showing up. Subjects then privately read through a set of computerized instructions describing the decision setting and familiarizing them with specific screen displays. While subjects were privately reading the set of computerized instructions, an overhead was also presented with summary information related to the private and group accounts. Finally, in the transition from one phase to the next, summary information regarding the subsequent decision setting was publicly projected on a large screen at the front of the lab and then read aloud by the experimenter.

The experimental design called for two replications of each of the six unique permutation orders of the three decision settings, excluding the alternative baseline. This led to twelve experimental sessions with 144 unique subjects. To investigate the potential framing effect associated with an unconditional external allocation to the group account, the remaining subject-motivation funds in our grant budget allowed two additional sessions utilizing the following ordering of decision settings: 1) alternate baseline, lump-sum matching, one-to-one matching, and 2) alternate baseline, one-to-one matching, lump-sum matching. Thus, the results reported below are drawn from a total of fourteen experimental sessions using 168 subjects to form 126 decision-making groups. Each group interacts over ten decision rounds resulting in a total of 1260 observations at the group level and 5040 observations at the individual level.

4. Experimental Results

Subject decisions are analyzed both graphically and econometrically at the group and individual level to examine the effects on allocations to the group account of changing the experimental setting. The analysis focuses on three performance measures. The first measure reported is the per-round token allocations to the group account by each four-person group, excluding any external matching allocations. The second performance measure is the per-round efficiency, where efficiency is defined as the percentage of maximum possible earnings extracted by the group.[6] The third performance measure is the per-round within-group dispersion of allocations to the group account. Specifically, the standard deviation about the mean group-member allocation is calculated.

4.A. Graphical Overview

[Figure 2 here]

[Figure 3 here]

[Figure 4 here]

Figures 2-4 display the mean value of each performance measure for each round pooled across experimental phases. Several very general observations can be made from these figures.

Observation 1*:* Mean allocations to the group account are highest in the lump-sum setting in all ten decision rounds, and lowest in the alternate baseline in eight of ten rounds.

Observation 2: Mean efficiency averaged over all ten decision rounds is lowest in the lump-sum setting, but the rank ordering across treatments varies from round to round.

Observation 3: Mean dispersion of group-account allocations within groups is lowest in the lump-sum setting in all ten decision rounds.

The lump-sum setting appears to be the most effective at generating allocations to the group account, as mean allocations in this setting are higher than all other settings for every round. In most rounds, however, average efficiency is lower in the lump-sum setting because of the severe penalty (loss of 60 tokens) if the threshold for the match is not reached.[7] This penalty

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[6] The formula for calculating per-round efficiency is $\frac{0.024*(\text{tokens}+\text{external tokens})+0.01*(100-\text{tokens})}{0.024*160}$, where "tokens" is defined as the aggregate internal token allocation to the group account and "external tokens" is defined as the tokens allocated to the group account by the external source. Because the external tokens are not provided by a subject within the experiment, the efficiency measure used in the analysis does not account for the value to the "external source" of unused tokens. This measure of efficiency is highly positively correlated with the total tokens (group + external) allocated to the group account in a round (r = 0.9829).

[7] Groups in the lump-sum setting failed to reach the threshold necessary for matching funds in 18.3% of all rounds.

is not as severe in the one-to-one setting; and the full match was always present in both baseline settings. The lump-sum setting also appears to diminish the end-game effect (i.e. decreasing allocations to the group account in Rounds 9 and 10) that is present in the other experimental settings. However, dispersion of group-account allocations within groups increases in Rounds 9 and 10 for all experimental settings.

4.B. Nonparametric Tests

This subsection presents two-tailed nonparametric tests to evaluate the validity of the above observations. Potential treatment-sequencing effects are also examined. The data to test *Observation 1* are the mean per-round allocation of tokens to the group account for each group (one observation per four-person group). Group means from all phases are included in each of the four samples (lump-sum, one-to-one, baseline, alternate-baseline), and these tests assume independence of group means within and across phases. A Kruskal-Wallis test rejects the joint null hypothesis that the data from all four settings are drawn from identical populations ($p = 0.018$). To further examine differences between experimental settings, a Wilcoxon rank-sum test is used for each setting pair. The null hypothesis of identical populations is rejected for the following pairs: lump-sum vs. baseline ($p = 0.032$, N=42, 36), lump-sum vs. one-to-one ($p = 0.024$, N=42, 42), and lump-sum vs. alternate-baseline ($p = 0.013$, N=42, 6). The other three pairs are not significantly different at the 10% significance level. Thus, the nonparametric tests support the observation that group-account allocations are highest under lump-sum matching.

The above analysis is repeated to test *Observation 2*. The data are the mean per-round efficiency for each group (one observation per group). A Kruskal-Wallis test fails to reject the joint null hypothesis that the data from all four samples are drawn from identical populations ($p = 0.5098$). Further, Wilcoxon rank-sum tests are not significant at the 10% level for any of the pair-wise comparisons. These nonparametric tests are thus not supportive of the *Observation 2* implication that efficiency is significantly lower, on average, under lump-sum matching relative to the other treatments. The insignificance of the rank-sum tests is not surprising, however, given the variation in efficiency rankings across rounds.

The data to test *Observation 3* are the mean per-round within-group standard deviation of group-account allocations (one observation per group). A Kruskal-Wallis test rejects the null hypothesis of the samples being drawn from identical populations at a 10% significance level ($p = 0.0973$). Wilcoxon rank-sum tests are also significant at the 10% level for both lump-sum vs. baseline ($p = 0.052$, $N = 42, 36$) and lump-sum vs. one-to-one ($p = 0.034$, $N = 42, 42$). All other setting pairs are not significant at the 10% level. These nonparametric tests offer marginal support for the observation that within-group allocation decisions tend to have lower dispersion under lump-sum matching.

The three-phase sequenced structure of the experiment may lead to differences in group-account allocations due to the particular phase in which the setting occurred. Therefore, in order to assess differences in group-account allocations between experimental settings, it is imperative to examine whether differences in allocations are related to the placement of a setting within a three-phase sequence. To examine the significance of sequence effects, Kruskal-Wallis tests are used. A test was completed for the baseline, lump-sum match, and one-to-one match setting.[8] The samples are constructed by calculating the mean group-aggregate per-round allocations of tokens to the group account for each sequencing history. For example, the seven samples used in the lump-sum matching test are: phase 1 ($N = 12$), phase 2 preceded by baseline ($N = 6$), phase 2 preceded by one-to-one matching ($N = 6$), phase 2 preceded by the alternate baseline ($N = 3$), phase 3 preceded by phase 1 lump-sum matching and phase 2 one-to-one matching ($N = 6$), phase 3 preceded by phase 1 one-to-one matching and phase 2 lump-sum matching ($N = 6$), and phase 3 preceded by phase 1 alternate baseline and phase 2 one-to-one matching ($N = 3$). The Kruskal-Wallis tests for each setting were not significant at the 10% level. Based on these nonparametric

[8] The alternate baseline setting occurred only in phase 1 of the two experimental sessions in which it was used.

tests, it appears that the sequence of the experimental settings does not contribute to differences in group -account allocations.

4.C. Regression Analysis

To further investigate the effect of decision settings, phases, and rounds on group-account allocations, an individual-specific fixed-effects regression model is estimated using all 5040 individual-level allocations to the group account. The individual-specific error components are estimated using the 30 decisions across all three phases for each of the 168 individual subjects. To account for lack of independence within a ten-round four-person group, clustered robust standard errors are utilized where the data are clustered by these 40 within-group decisions.[9] The regression equation is:

$$y_{i,p,r} = \alpha_i + \mathrm{x}'_{i,p,r}\beta + u_{i,p,r}\,,\ i = 1,2,...,168\,,\ p = 1,2,3\,,\ r = 1,2,...,10. \qquad (1)$$

The i,p,r subscripts index individuals, phases, and rounds, respectively. The dependent variable, $y_{i,p,r}$, is the allocation to the group account, α_i is the individual-specific fixed-effect vector, and **x** is the data matrix of independent variables: a lump-sum matching dummy variable (LUMP), a one-to-one matching dummy variable (1TO1), an alternative-baseline dummy variable (ALTBASE), two treatment-phase dummy variables (PHASE2 and PHASE3), and nine decision-round dummy variables (RNDr, r=2, 3, … ,10). The usual idiosyncratic residual error vector is $u_{i,p,r}$.

[Table 2 here]

Table 2 displays the coefficient point estimates, clustered robust standard errors, and two-tailed significance tests of the coefficients. In support of *Observation* 1, the table reveals that lump-sum matching generates a significant increase in tokens allocated to the group account relative to the original no-matching baseline; however, the smaller increase generated by one-to-

[9] For a detailed discussion of the heteroskedasticity-robust Huber/White sandwich estimator of variance in panel-data models see, for example, Cameron and Trivedi (2005, Chapter 21, Section 21.2.3). The specific implementation utilized here is documented in Rogers (1993).

one matching is not significantly different from the original baseline. As expected, the ALTBASE coefficient is negative; removing the "external source" frame from the baseline group-account earnings function tends to reduce group-account allocations. This difference is significant. Wald tests result in rejection of the following pair-wise null hypotheses: LUMP = 1TO1 (p = 0.000), LUMP = ALTBASE (p = 0.000), and 1TO1 = ALTBASE (p = 0.000).[10] Thus, allocations to the group account are significantly higher in the lump-sum setting than either the alternate baseline or the one-to-one setting. Further, allocations in the one-to-one setting are significantly higher than the alternate baseline setting. While the primary focus here is on the effects of altering the experimental decision setting, note that the treatment-phase dummies are not significant but there are significant differences across decision rounds. In particular, relative to round 1, group-account allocations tend to be slightly higher on average in rounds 2-4 and there is a significant drop in group-account allocations in the final two rounds. Referring back to Figure 2, this end-game drop in allocations is evident in all except the lump-sum setting.

[Table 3 here]

The conclusions from the individual fixed-effects model are also supported when the group-account allocations are analyzed at the group level. Table 3 reports estimates from a random-effects regression model using all 1260 group-level observations where tokens allocated

[10] Two additional models were also estimated .The results reported in Table 2 are robust to these alternative model specifications. The first model is an individual-specific random-effects model using only phase-1 data. As in Table 2, cluster-robust standard errors are utilized with allocation decisions clustered by the forty within-group observations. The random-effects estimator is necessary since all three experimental setting dummy variables are round invariant, removing the possibility of using the fixed-effects estimator. Estimation of the phase-1 random-effects model results in only one minor deviation from the results reported in Table 2: a Wald test for the null hypothesis 1TO1 = ALTBASE is significant at the 10% level (p = 0.0803). The second model is a two-limit censored-normal (Tobit) regression model with group-level clustered standard errors. This model makes strict distributional assumptions to account for the observations that occur at the fixed boundaries of group account allocations. Approximately 31.3% of the observations on the dependent variable (1579 of 5040) occur at the fixed upper boundary of 25 tokens to the group account, and 10.3% of the observations (518 of 5040) occur at the lower boundary of zero. The estimates of the Tobit regression are similar in sign and magnitude to the estimates reported in Table 2. The significance of the setting dummy variables and pair-wise Wald tests from the Tobit model result in two deviations from the fixed-effects model: ALTBASE is not significant at the 10% level, and the Wald test for 1TO1 = ALTBASE is significant at the 10% level (p = 0.056).

to the group account by a four-person group (the aggregate allocation excluding external tokens) is the dependent variable.[11] The regression equation is:

$$y_{g,p,r} = \alpha + \mathrm{x}'_{\mathrm{g,p,r}}\beta + u_{g,p,r} + \varepsilon_g \,,\; g = 1,2,\ldots,42 \,,\; p = 1,2,3 \,,\; r = 1,2,\ldots,10. \qquad (2)$$

The g,p,r subscripts index four-person groups, phases, and rounds, respectively. The independent variables are identical to those described for equation 1 and reported in Table 2. The usual idiosyncratic residual error vector is $u_{g,,p,r}$, and ε_g is a group-specific error component. Cluster-robust standard errors are utilized with observations clustered by experimental sessions (nine groups across the three phases) to account for possible lack of independence across groups within a session. The results reported in Table 3 are very similar to those using the group-account allocations at the individual level that were reported in the fixed-effect model. Specifically, lump-sum matching significantly increases group-account allocations relative to the baseline setting; however, allocations in the one-to-one setting are not significantly different from the baseline. The alternate baseline significantly reduces allocations relative to the baseline. Further, the following null hypotheses are rejected using Wald tests: LUMP = 1TO1 ($p = 0.000$), LUMP = ALTBASE ($p = 0.000$), 1TO1 = ALTBASE ($p = 0.013$). Therefore, allocations at the group level are greatest in the lump-sum setting. The one-to-one setting does not significantly increase allocations relative to the baseline, while allocations are lowest in the alternate baseline.[12]

[Table 4 here]

The efficiency and allocation-dispersion performance measures also require analysis at the group level. First, efficiency is considered. The regression model described in equation (2) is repeated using a group's per-round efficiency as the dependent variable. Table 4 displays the regression coefficients, robust standard errors, and two-tailed significance tests for the coefficients. In support of *Observation* 2, the table reveals that lump-sum matching results in a

[11] Random effects are needed because the experimental settings variables are round invariant for groups.
[12] The significance of all Wald tests reported for the random-effects model in Table 3 is upheld when the model is estimated using only phase-1 data, which avoids the possible lack-of-independence complication for phase-2 and phase-3 groups within a three-phase experimental session.

small but significant (p = 0.007) decrease in efficiency compared to the baseline. Average efficiencies in the other settings are also significantly decreased from the baseline. Despite the differences in penalties from failing to reach the full match, a Wald test comparing the pair-wise null hypothesis of LUMP = 1TO1 is not rejected (p = 0.278). One reason for the lack of significance between efficiencies in the lump-sum setting and the one-to-one setting is that there were substantially more full matches in the lump-sum setting compared to the one-to-one setting (81.7% of all rounds compared to 61.4% of all rounds, respectively). Again, an end-game effect is present; efficiency decreases by an average of 3% in round 9 and 4% in round 10 when compared to round 1. This result is consistent with Figure 3, which displays a decrease in efficiency for the final two rounds in all environments but lump-sum matching.[13]

[Table 5 here]

The third performance measure to analyze is the dispersion of within-group allocations to the group account, where dispersion is calculated by the standard deviation about the mean individual allocation to the group account. The regression model described in equation (2) is estimated using per-round standard deviation of group-member allocations as the dependent variable. Table 5 displays the regression coefficients, robust standard errors, and 2-tailed significance tests for the coefficients. In support of *Observation* 3, the table reveals that the lump-sum setting results in a significant (p = 0.019) decrease in dispersion compared to the baseline setting. Wald tests reject the pair-wise null hypotheses of LUMP = 1TO1 (p = 0.000) and LUMP = ALTBASE (p = 0.078). A Wald test comparing the remaining pair-wise null hypothesis (1TO1 = ALTBASE) is not significant at the 10% level. Thus, dispersion in group

[13] Two robustness checks for the random-effects model of efficiency were completed. First, the random-effects model was estimated using only phase-1 data. Tests of the significance of the coefficient estimates were qualitatively similar to the results presented in Table 4 with one exception. The null hypothesis of LUMP = BASELINE was not rejected at the 10% significance level. Second, a two-limit censored-normal (Tobit) model employing clustered standard errors at the group level was also estimated to account for the observations at the boundaries of the decision space. Thirty-one of 1260 observations are at the upper efficiency limit (1) and one observation is at the variable lower efficiency limit. (The lower efficiency limits in the baseline and alternate baseline are larger than the lower limit in the matching environments.) All tests of significance were qualitatively similar to those shown in Table 4.

account allocations is significantly less in the lump-sum setting than either the one-to-one setting or the alternate baseline. As can be seen in Figure 4, dispersion increases during the final three rounds in each setting. This observation is supported by the regression results, as RND8 ($p = 0.055$), RND9 ($p = 0.000$), and RND10 ($p = 0.000$) are all positive and significant.[14]

4.D. Individual Allocations to the Group Account: Benchmark Frequencies

[Figure 5 here]

This subsection analyzes group-account allocations at the individual level organized around the frequency of occurrence of three benchmark allocations: the individual maximum (25 tokens), the symmetric Nash equilibrium (15 tokens), and complete free riding (0 tokens).[15] To avoid any possible impact on token allocations from an individual's participation in multiple decision settings, only the phase-one data are examined. Figure 5 displays relative frequencies of these benchmark allocations for each experimental decision setting, pooling across all ten decision rounds.[16] The percentage of occurrences of the maximum allocation is somewhat higher in the matching settings relative to the baseline settings. Further, the lump-sum setting results in more allocations that are consistent with the symmetric Nash equilibrium compared to the one-to-one setting, and complete free riding occurs less frequently under lump-sum matching relative to the other three settings.

To formally examine the significance of these informal observations, negative binomial count-data regressions are performed where the dependent variable is the number of rounds that an individual submitted a specific benchmark allocation (an integer between 0 and 10). The independent variables are the LUMP, 1TO1, and ALTBASE dummy variables described at the

[14] Using only phase-1 data in the random-effects allocation-dispersion model results in the null hypothesis of 1TO1 = BASELINE being rejected at the 5% level ($p = 0.019$). The other tests are qualitatively similar to the results reported above and in Table 5.

[15] Allocations near the symmetric Nash equilibrium ($14 \leq \text{tokens} \leq 16$) were also examined. The results were very similar to those of the symmetric Nash equilibrium.

[16] Note that the symmetric Nash equilibrium only applies to lump-sum matching and one-to-one matching. The unique Nash equilibrium allocation to the group account is zero tokens for each baseline environment.

beginning of section 4.C.[17] Because each individual is part of a four-person group, an individual's token allocations are likely to be influenced by the previous allocations of other group members. To account for this within-group dependence, robust clustered standard errors are reported where observations are clustered by decision groups.

[Table 6 here]

[Table 7 here]

[Table 8 here]

The regression results for each benchmark allocation appear in Tables 6, 7, and 8. A convenient way to interpret the regression coefficients in the negative binomial model is to examine incidence-rate ratios (IRR), where $\text{IRR} = e^{\beta_i}$. IRRs reveal the percentage change in the expected count of a benchmark allocation due to a change in the treatment condition, holding all other independent variables constant. For example, in Table 6, the lump-sum setting increases the expected frequency for the maximum allocation by a multiple of 1.18 compared to the baseline setting, an 18% increase [i.e. 100*(IRR – 1)]. Overall, however, Table 6 shows the regression model is not significant when the maximum allocation count is used as the dependent variable ($p = 0.251$). Table 7 shows that the coefficient for the LUMP dummy is positive and significant ($p = 0.009$); the IRR indicates a 79% increase over one-to-one matching in the expected number of rounds where the symmetric Nash equilibrium allocation is submitted. Finally, Table 8 shows that the coefficient for the LUMP dummy is negative and marginally significant ($p = 0.064$); the IRR indicates a 62% decrease in the number of complete free-riding rounds relative to the baseline level. Wald tests of null hypotheses LUMP = 1TO1 ($p = 0.0112$) and LUMP = ALTBASE ($p = 0.0634$) are significant. The remaining pair-wise null hypothesis, 1TO1 = ALTBASE, is not rejected at the 10% level. Thus, significantly less free riding occurs in

[17] A Poisson regression model was estimated first, but the results indicated that the assumption of equidispersion (equality of the mean and variance inherent in a Poisson process) must be rejected. Following Long (Chapter 8, 1997) and Cameron & Trivedi (Chapter 20, 2005) the negative binomial model was utilized to capture overdispersion in the dependent variable.

the lump-sum setting than either the one-to-one or the alternate baseline setting. In summary, lump-sum matching appears to: 1) significantly increase the frequency of individual allocations consistent with the symmetric Nash equilibrium relative to one-to-one matching, and 2) significantly decrease the frequency of complete free-riding allocations relative to the other decision settings examined here.

5. Summary and Conclusions

In the experimental literature on the voluntary provision of public goods, a wide range of studies examine alternative institutional arrangements intended to reduce collective action problems by creating incentives that facilitate cooperation. The research reported in this study adds to this literature by examining behavior in two fund-raising institutions found commonly in the field: lump-sum matching and one-to-one matching, where matching funds are provided by an "external" donor.

The experimental results reveal higher "internal" (within-group) resource allocations to the public good under lump-sum matching. An explanation supporting this result is that missing the threshold required to provide the full match results in a larger earnings loss in the lump-sum setting when compared to the one-to-one setting. Internal allocations in the lump-sum setting are also less dispersed, with more individual allocations at or near the symmetric Nash equilibrium prediction and fewer individual allocations consistent with complete free riding. Neither the lump-sum nor the one-to-one setting provides strong support for play of the symmetric Nash equilibrium. Finally, although lump-sum matching leads to greater internal allocations to the public good, there is not a significant difference in efficiency between the two matching-funds settings due to decision rounds where groups under lump-sum matching do not reach the threshold and thus receive no matching funds. In the experimental settings investigated here, external matching funds that are not extracted by a four-person group are wasted rather than being carried over to future decision rounds. In naturally-occurring field settings the validity of this rather harsh component of the experimental environment is doubtful. To the extent that unused matching funds are transferred to future endeavors that augment the provision of the public good, the efficiency comparisons reported here are of less relevance than the comparison of internal resource allocations to the public good.

As a methodological issue, it is interesting to note the behavioral response to the framing change made between the baseline and alternate baseline settings. The alternate baseline removed any wording that alluded to allocations to the group account from an external source. Instead, the additional tokens were simply added to payoffs from the group account by adjusting the intercept term of the group account return function. With this framing change, lower allocations to the group account were observed in the alternate baseline. Although based on a small sample size, this result is supportive of similar results that examine leadership contributions to a public good.

Fund-raising drives suggest several other interesting extensions to the experiments reported here. In particular, in field applications organizations often provide information on the current status of the fund drive with respect to donations. Future research will examine this issue, using both lump-sum and one-to-one matching, by giving subjects intra-round information on the current aggregate allocation to the public good in conjunction with intra-round updating of individual allocation decisions. An "increase-only" allocation rule can be applied to intra-round updates of individual decisions. Larger group sizes, other group-account earnings structures, and the use of nonmonetary rewards will also be investigated.

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Table 1. Characteristics of Decision Settings

	Baseline	Lump-Sum Matching	One-to-One Matching
Individual Token Endowment Per-Round	25	25	25
Decision Rounds	10	10	10
Per-Token Return to Private Account	\$0.01	\$0.01	\$0.01
Individual Per-Token Return from Group Account	\$0.006	\$0.006 for tokens other than the 60th token	\$0.012 for tokens 1-60, \$0.006 for tokens 61 and above
Total Individual Earnings: All Tokens to the Private Account	\$6.10	\$2.50	\$2.50
Total Individual Earnings: Symmetric Nash Equilibrium of 15 tokens	NA	\$8.20	\$8.20
Total Individual Earnings: All Tokens to the Group Account	\$9.60	\$9.60	\$9.60

Table 2. Fixed-Effects Model: Individual Allocations to Group Account

Independent Variable	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 t	p-value
CONSTANT	15.5658	0.5166	30.13	0
LUMP	2.2728	0.4582	4.96	0
1TO1	0.6258	0.4815	1.3	0.196
ALTBASE	-2.1035	0.8251	-2.55	0.012
PHASE2	-0.6439	0.4529	-1.42	0.158
PHASE3	-0.6575	0.4203	-1.56	0.12
RND2	1.0119	0.2945	3.44	0.001
RND3	0.6706	0.3523	1.9	0.059
RND4	0.8512	0.3648	2.33	0.021
RND5	0.3234	0.3870	0.84	0.405
RND6	-0.0337	0.3882	-0.09	0.931
RND7	0.0218	0.3846	0.06	0.955
RND8	-0.1310	0.3747	-0.35	0.727
RND9	-0.7044	0.3908	-1.8	0.074
RND10	-1.2480	0.4464	-2.8	0.006

Total Number of Observations = 5040 = 126 clusters of 40 observations
Model: F(14,125) = 9.04, p = 0.000
Fraction of variance due to fixed effect: 0.442

Table 3. Random-Effects Model: Group Allocations to Group Account

Independent Variable	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT	62.1570	1.9186	32.40	0.000
LUMP	9.1312	1.3950	6.55	0.000
1TO1	2.5431	1.6925	1.50	0.133
ALTBASE	-7.8864	4.1244	-1.91	0.056
PHASE2	-2.4819	1.3498	-1.84	0.066
PHASE3	-2.5605	1.6786	-1.53	0.127
RND2	4.0476	1.0352	3.91	0.000
RND3	2.6825	1.3671	1.96	0.05
RND4	3.4048	1.4013	2.43	0.015
RND5	1.2937	2.1349	0.61	0.545
RND6	-0.1349	1.6227	-0.08	0.934
RND7	0.0873	1.5262	0.06	0.954
RND8	-0.5238	1.4321	-0.37	0.715
RND9	-2.7381	1.5577	-1.76	0.079
RND10	-4.9921	1.1084	-4.50	0.000

Total Number of Observations = 1260 = 14 clusters of 90 observations
Fraction of variance due to session-specific random effect: 0.565

Table 4. Random-Effects Model: Group Efficiency

Independent Variable	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT	0.8771	0.0127	69.13	0.000
LUMP	-0.0351	0.0129	-2.72	0.007
1TO1	-0.0224	0.0104	-2.15	0.032
ALTBASE	-0.0340	0.0160	-2.12	0.034
PHASE2	-0.0170	0.0087	-1.96	0.050
PHASE3	-0.0171	0.0157	-1.09	0.276
RND2	0.0204	0.0077	2.66	0.008
RND3	0.0074	0.0119	0.62	0.534
RND4	0.0055	0.0096	0.57	0.567
RND5	-0.0091	0.0177	-0.51	0.608
RND6	-0.0134	0.0130	-1.03	0.303
RND7	-0.0149	0.0119	-1.25	0.212
RND8	-0.0141	0.0120	-1.17	0.242
RND9	-0.0286	0.0148	-1.94	0.052
RND10	-0.0396	0.0124	-3.20	0.001

Total Number of Observations = 1260 = 14 clusters of 90 observations
Fraction of variance due to session-specific random effect: 0.342

Table 5. Random-Effects Model: Within-Group Standard Deviation of Individual Allocations to Group Account

Independent Variable	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT	7.5268	0.4048	18.59	0.000
LUMP	-1.0568	0.4517	-2.34	0.019
1TO1	0.0677	0.4557	0.15	0.882
ALTBASE	0.4012	0.8684	0.46	0.644
PHASE2	-0.1001	0.3629	-0.28	0.783
PHASE3	-0.1227	0.3140	-0.39	0.696
RND2	-0.2424	0.1835	-1.32	0.187
RND3	0.1594	0.2473	0.64	0.519
RND4	0.0351	0.2892	0.12	0.903
RND5	0.3553	0.2554	1.39	0.164
RND6	0.4774	0.2734	1.75	0.081
RND7	0.4384	0.2840	1.54	0.123
RND8	0.5451	0.2846	1.92	0.055
RND9	1.3791	0.2933	4.70	0.000
RND10	1.6131	0.3266	4.94	0.000

Total Number of Observations = 1260 = 14 clusters of 90 observations
Fraction of variance due to session-specific random effect: 0.496

Table 6. Count-Data Model: Maximum Allocation

Independent Variable	IRR	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT		1.0488	0.0908	11.56	0.000
LUMP	1.1825	0.1676	0.1515	1.11	0.269
1TO1	1.2482	0.2217	0.1487	1.49	0.136
ALTBASE	0.9051	-0.0997	0.1790	-0.56	0.578

Total Number of Observations = 168 = 42 clusters of 4 observations
Model: $\chi 2(3) = 4.10$, $p = 0.251$

Table 7. Count-Data Model: Symmetric Nash Equilibrium Allocation

Independent Variable	IRR	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT		-0.2336	0.1693	-1.38	0.168
LUMP	1.7895	0.5819	0.2227	2.61	0.009

Total Number of Observations = 96 = 24 clusters of 4 observations
Model: $\chi 2(1) = 6.83$, $p = 0.009$

Table 8. Count-Data Model: Complete Free-Riding Allocation

Independent Variable	IRR	Coefficient Estimate	Robust Clustered Standard Error	Ho: Coefficient = 0 Z	p-value
CONSTANT		-0.1335	0.2902	-0.46	0.645
LUMP	0.3810	-0.9651	0.5205	-1.85	0.064
1TO1	1.3333	0.2877	0.3758	0.77	0.444
ALTBASE	1.1429	0.1335	0.4978	0.27	0.789

Total Number of Observations = 168 = 42 clusters of 4 observations
Model: $\chi 2(3) = 6.54$, $p = 0.088$

Figure 1. Group-Account Earnings for the Decision Settings

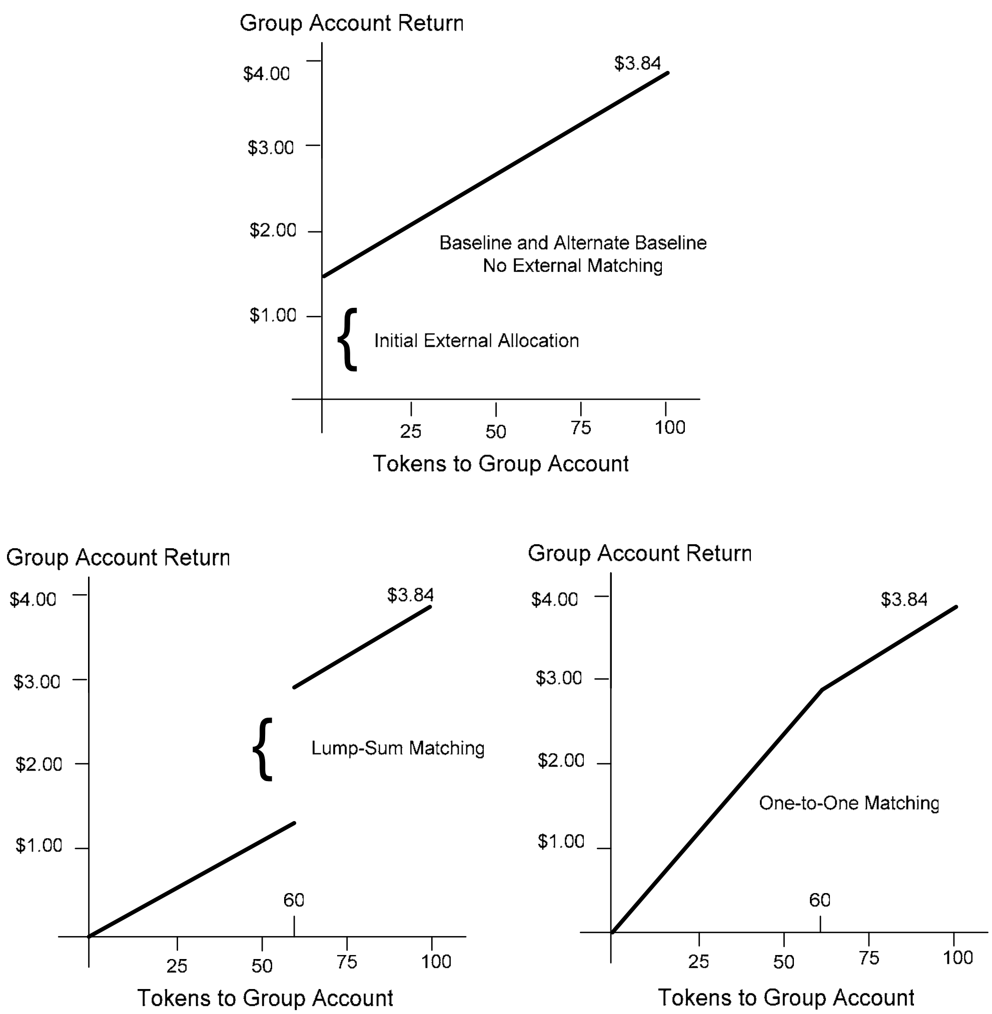

Figure 2. Mean Internal Token Allocation to the Group Account

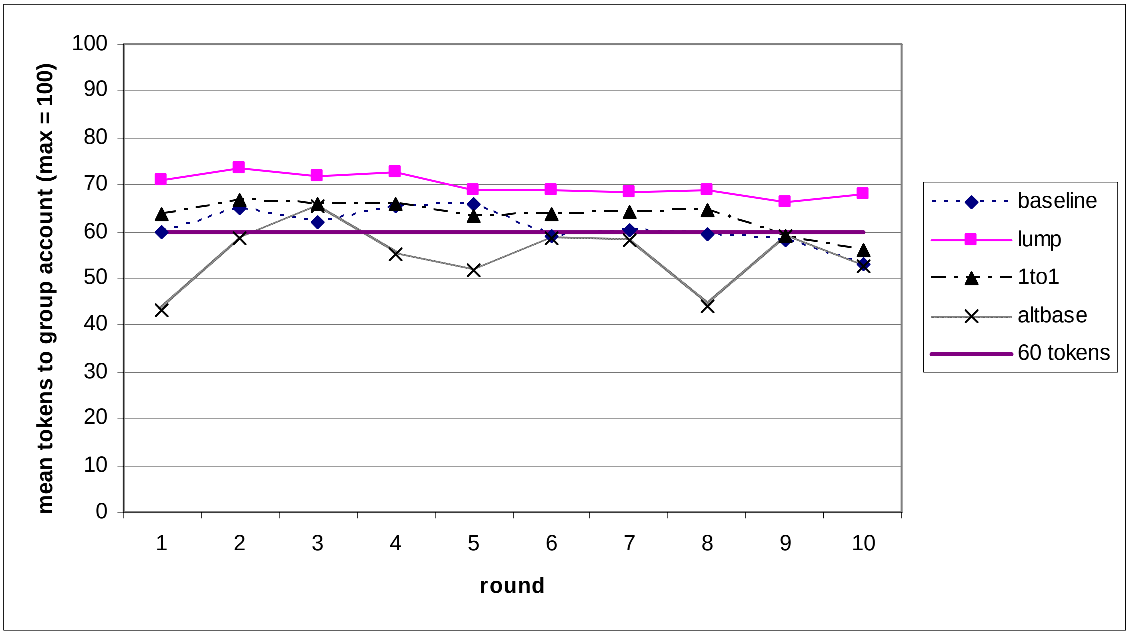

Figure 3. Mean Efficiency

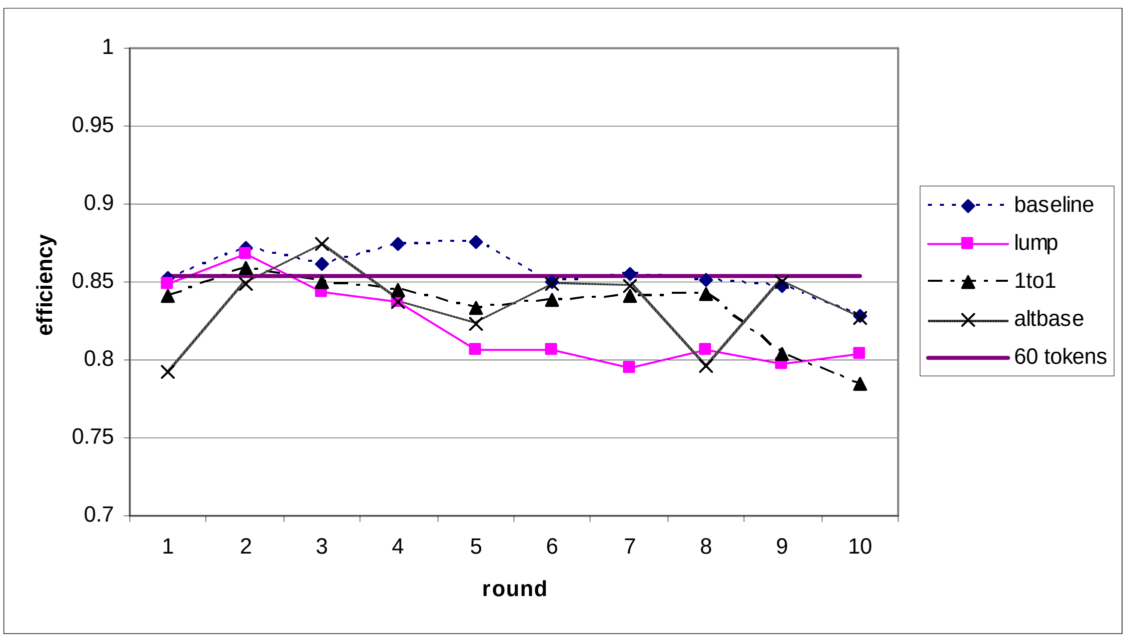

Figure 4. Mean Standard Deviation of Tokens to the Group Account

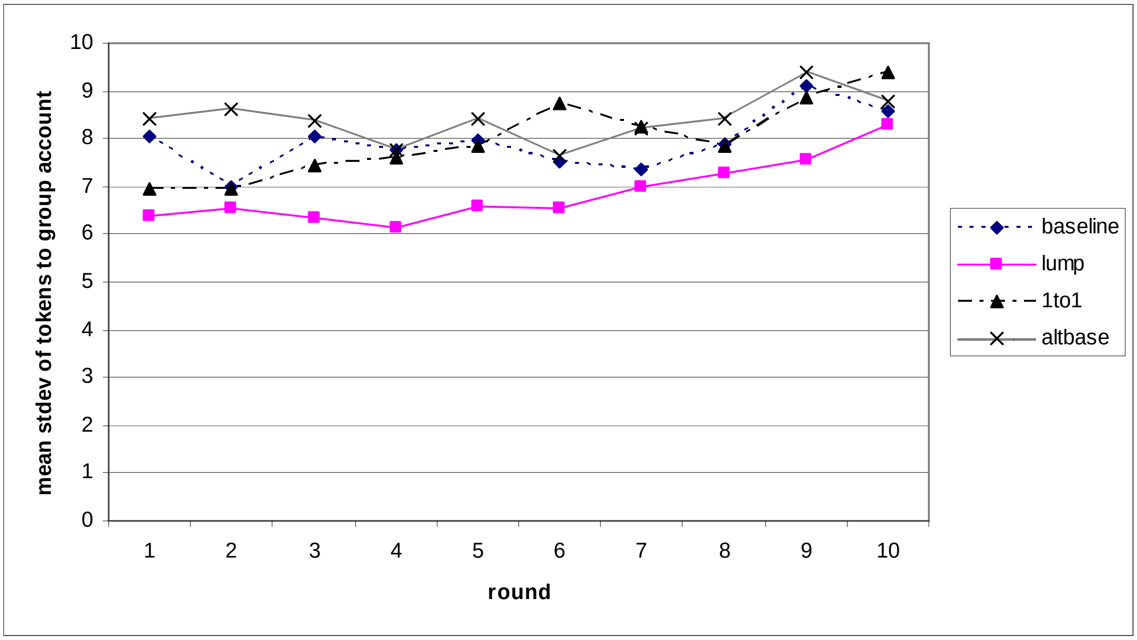

Figure 5. Individual Token Allocations to the Group Account: All Rounds, Phase 1